Response to the comments by anonymous referee #1 on manuscript WES-2026-100

Turbulence characterization in near-coastal environment using triple short-range lidars and mast anemometry

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Note: Line numbers cited by the referee refer to the WESD preprint, whereas line numbers cited in the response refer to the revised manuscript.

1. General comment

Referee comment

This paper presents measurements of turbulence spectra and coherence at a site in North Germany using a met-mast equipped with cup and sonic anemometers as well as with three synchronized ground-based lidars. The authors find that the spectra and coherence from the different instruments agree well with each other. More importantly, they are able to reproduce and verify observations presented in previous publications. They also present an empirical model to capture vertical and lateral coherences that could be useful for generating wind fields along with the IEC Kaimal model.

I think their methods and analysis are scientifically sound, and I would like to praise the quality of presentation of the results. However, I believe a lot more work is needed before this article is ready for publication. My main concern is about the environment, which the measurements are intended to characterize. I have substantiated this below, along with other technical criticisms. I also have some comments regarding the writing, please find them after the "technical comments".

We thank the reviewer and appreciate the feedback and constructive comments on our manuscript. We have addressed the individual comments below and updated the manuscript accordingly.

2. Technical comments

Referee comment

Abstract: Clear and well-written

We appreciate the positive feedback on the abstract.

Referee comment

Line 61: I do not agree with the claim that this study addresses the limitation of Patel et al. 2026 which lacked information about the vertical component. This is because the measurements in this study are taken onshore (although near the coast) whereas the limitation of Patel et al. stemmed from the difficulty of measuring the w-component offshore using a lidar. Indeed, there is no technically feasible solution for this problem as of yet, so the sentence in Line 61 can be a little misleading.

We agree that the lidar setup in the present study technically does not address the limitation of Patel et al. (2026) due to the differences between offshore and onshore environments. We have therefore removed the claim and have updated the manuscript as follows:

Changes to manuscript

Line 62:
In this study, coherence is characterized at a near-coastal site using a triple-lidar system that enables reconstruction of the full three-component wind field [...].

Referee comment

Line 89, 108 and Figure 1, 2: It is claimed that the sector between 315 to 345° experiences "offshore" flow. But looking at the map of the site in the two figures, I'm not sure if this is the case. It looks like the site is about 2km inland. Using the rule of thumb that the internal boundary layer (IBL) depth grows as x/10 m where x is the downstream distance from the roughness change. So, in this case, the measurements (which are taken at z = 25 to 125m) would be within the IBL and not really offshore.

We agree that the terms "offshore flow" and "offshore sector" are not ideal to describe the wind approaching the test site from the estuary and have rephrased the relevant sections.

Changes to manuscript

Line 89:
The Weser estuary lies northwest of the test site, resulting in a sector of approximately 30° where the flow reaches the site after traversing the sea surface.

Line 109:
A wind rose for this period is shown in Fig. 4, indicating both the estuary (315°...345°) and the grassland southwest of the test site (220°...250°) as the prevailing wind directions.

Line 284:
Unstable conditions account for roughly 20 % overall, but increase to about 50 % within the northwestern sector with flow from over the estuary.

Referee comment

I would like to raise another point here which is that I do not understand what wind direction sectors are used to calculate the spectra in Figures 12, 13, 14 and the coherence in Figures 15, 16, 17. Do you use all wind directions apart from the sectors where the measurements are influenced by the turbine or the mast wakes (as I can infer from Table 1)? Then this is not quite correct as I would expect the flow from north-west to have different characteristics than the flow from the eastern sector. Moreover, since the article is intended to characterize "near-coastal" flow, more care should be taken when selecting which wind directions to include in the analysis of spectra and coherence because flow from many directions at this site is probably not coastal. If the required processing of wind directions has already been done, then please explain it in the manuscript as it is not clear in the current form.

The spectral estimates presented in Fig. 12–17 are averaged over all wind directions after excluding sectors potentially affected by turbine or mast wake interference. Prior to ensemble averaging, the auto-spectra are normalized by their respective variances to account for directional inhomogeneities in turbulence intensity.

We agree with the reviewer that the spectral shape itself is also expected to vary with wind direction. To address this point, we have added a new figure (Fig. 15) showing the normalized auto-spectra under neutral conditions for the four wind-direction sectors previously compared in terms of integral length scales. The results are consistent with the length scale analysis, showing increased low-frequency energy for the western and northwestern sectors.

However, the directional dependence is less pronounced than the variation across atmospheric stability, which is the primary focus of this study and the main reason why ensemble averages over all valid wind directions are presented in the remaining figures. As noted by the reviewer in an earlier comment, even the "offshore sector" is influenced by the development of an internal boundary layer, causing a deviation from pure offshore flow. This likely explains the relatively modest differences observed between the spectra obtained for varying sectors.

We acknowledge that averaging across all wind directions combines flow over different upstream terrain conditions. For this reason, we deliberately use the term "near-coastal" rather than "coastal" to describe the site. Wind turbines operating at such locations are exposed to inflow conditions that vary between sectors; however, based on the present analysis, the influence of atmospheric stability on the turbulence characteristics is found to be more significant than the directional variations associated with the differences in terrain.

Changes to manuscript

Added Fig. 15 plus discussion:

Line 397:

The auto-spectral estimates presented in Fig. 12–14 are ensemble averages over all wind directions that are not affected by turbine or mast wake interference. Prior to ensemble averaging, each spectrum is normalized by its corresponding variance to reduce the influence of directional differences in turbulence intensity. However, as shown in Fig. 10d–f, the integral length scales vary with wind direction, indicating that the spectral shape is also expected to differ based on the upstream terrain. This effect is examined in Fig. 15, which presents normalized spectra under neutral conditions for four wind-direction sectors

Changes to manuscript (continued)

with contrasting terrain characteristics. As the upstream roughness decreases from Fig. 15a (residential area) to Fig. 15d (river estuary), the low-frequency energy increases, causing the along-wind turbulence peak around n = 0.1 to gradually broaden into a spectral plateau. This behaviour is consistent with the corresponding increase in the integral length scales discussed previously. A similar increase in low-frequency energy is observed in the cross-wind spectra, whereas the vertical velocity spectra are comparatively insensitive to the upstream terrain. Overall, the sonic anemometer and SRWS spectra exhibit similar trends across the different sectors. Some estimates, however, such as the sonic spectrum at z = 110 m in Fig. 15c, show increased uncertainty due to the smaller number of available samples.

Referee comment

Equation 1 (Line 115): How is the azimuth defined? As in which direction corresponds to α = 0? In my experience, the azimuth of the instruments is defined with respect to True North and then Equation 1 would contain the east-west, north-south and vertical wind components on the LHS. But you have defined it with the x, y and z components. Since the x-direction in Figure 3(b) is not aligned with True North, it seems to me Equation 1 is incorrect, unless you have defined the azimuth with respect to the x-direction.

We thank the reviewer for this observation. We have added that the azimuth is defined with respect to the x-direction, not to True North.

Changes to manuscript

Line 117:
Here, the azimuth angle is defined with respect to the x-axis in Fig 3b.

Referee comment

Line 124: Why was the 30 min period chosen? I imagine you wanted to analyze the spectra below the commonly used 1/10min frequency, but why did you not go even lower than 30 min?

The choice of a 30-minute averaging period represents a compromise between extending the spectral range towards lower frequencies and maintaining the assumption of stationarity. While longer averaging periods would allow to resolve the spectra at even lower frequencies, they also increase the likelihood of changes in atmospheric conditions within a sample, particularly with respect to atmospheric stability. Since one of the main objectives of this study is to investigate the influence of stability on turbulence characteristics, preserving stationarity within each averaging period is an important consideration. A 30-minute averaging period was found to capture the low-frequency motions of interest while providing a substantially larger number of stationary samples than would be obtained using longer averaging periods such as 1 hour.

Referee comment

Figure 4: I would suggest making another wind rose showing the directions that you have analyzed in this study.

We agree that a wind rose showing only the wind directions included in the analysis would be a valuable addition if the study were restricted to specific sectors. However, as noted in our response to an earlier comment, the analysis includes all wind directions except for the relatively narrow sectors potentially affected by turbine wake or mast interference. Since these excluded sectors differ slightly between the mast and lidar datasets, we believe that the existing wind rose, together with Table 1, provides the necessary information without introducing an additional figure.

Referee comment

Line 194-195: Do the authors not correct for the phase delay that comes from the longitudinal separation (as they have done so for the time lag in the scanning pattern in Equation 9)?

The lateral coherence estimates are indeed corrected for both the phase delay introduced by the scanning sequence and the additional phase delay resulting from the longitudinal separation of the sampling volumes. In Eq. (9), the total time lag τ represents the sum of these two contributions. We understand that this was not sufficiently clear in the original manuscript and have revised the text to explicitly state this.

Changes to manuscript

Line 202:
For lateral coherence, horizontal wind directions non-normal to the bow-tie plane introduce an additional time shift τ_ε due to the longitudinal separation d_x resulting from the yaw misalignment ε. The total time lag τ used for the phase correction is therefore given by the sum of the scanning-induced delay τ_{scan} and the longitudinal delay τ_ε, that is estimated as:

Referee comment

Figure 10: I find it surprising that in (a) the length scales are larger during neutral conditions than under very unstable conditions. Can the authors comment on why this might occur?

We agree that this observation is not intuitive and suspect that it reflects limitations of Taylor's hypothesis of frozen turbulence under strongly unstable conditions. We have added a short discussion on this to the manuscript:

Changes to manuscript

Line 308:

The dependence of the integral length scales on atmospheric stability and wind direction is illustrated in Fig. 10. Under neutral conditions, the integral length scales are within the ranges of 100 to 250 m for L_u, 30 to 100 m for L_v, and 10 to 50 m for L_w. From neutral to weakly unstable conditions, the length scales increase, consistent with enhanced turbulence generation due to buoyancy. Unexpectedly, this trend does not persist in the strongly convective regime, which may be related to limitations of Taylor's hypothesis of frozen turbulence. The estimation of integral length scales from single-point temporal auto-correlation functions assumes that turbulent structures are advected past the measurement location without undergoing significant evolution. Under unstable stratification, buoyancy-driven turbulence may accelerate the temporal evolution of turbulent eddies, causing the measured auto-correlation functions to decay more rapidly. Consequently, the slightly smaller integral length scales estimated under strongly unstable conditions may not necessarily reflect smaller turbulent structures, but rather indicate departures from Taylor's hypothesis of frozen turbulence.

From neutral to weakly stable conditions, all length scales decrease substantially as buoyancy transitions from enhancing to suppressing turbulence.

Referee comment

Line 313: It is not clear what is meant by an ensemble and what samples the ensemble-average is taken over. This is related to my previous comment about **Line 89**, **108** and **Figure 1, 2**.

We thank the reviewer for pointing this out. We agree that the definition of the ensemble averages was not sufficiently clear in the original manuscript. As discussed in our response to the previous comment, we have clarified that the ensemble averages are computed from all accepted 30-min samples within the respective atmospheric stability class, excluding only wind directions affected by turbine wake or mast interference. As detailed in the previous response, we have revised the manuscript accordingly and added a discussion of the influence of wind direction, including a new figure showing the directional dependence of the neutral auto-spectra.

Referee comment

Line 372 to 376 and Figure 14: Please mention from which height mesoscale turbulence is visible in the spectra. From (d) and (i) it looks as if the spectra rise at the lowest frequencies even for z = 55m which is surprising.

The increase in low-frequency energy is indeed observed at all measurement heights, including 25 m and 55 m. Although the effect becomes more pronounced with increasing height, it is not confined to the upper measurement levels. Similar observations were reported by Cheynet et al. (2018) at the FINO1 site, where enhanced low-frequency energy extended down to approximately 40 m. We agree that attributing these motions solely to mesoscale motions is not sufficiently supported by the present measurements. We have therefore revised the relevant sections to use the more general term "low-frequency motions" instead. While mesoscale turbulence is one possible explanation, the observed spectral enhancement may also be associated with other stable boundary layer phenomena, such as buoyancy oscillations near the Brunt–Väisälä frequency.

Changes to manuscript

Line 346:
Simultaneously, low-frequency contributions emerge, likely related to mesoscale motions.

Line 369:
Under neutral conditions, the w spectra follow the basic $f\,S_i(n) \propto f^1$ and $f\,S_i(n) \propto f^{-2/3}$ scaling, as neither a plateau nor a low-frequency contribution is present.

Line 391:
The low-frequency contribution captured within the 30-minute averaging window is generally well approximated by the $S_i(n) \propto f^{-5/3}$ scaling.

Line 541:
Under stable stratification, the spectral gap becomes more pronounced as additional low-frequency energy emerges in the horizontal components, potentially related to mesoscale motions.

Referee comment

Figure 19: The measured coherence for the largest separations (120 and 160 m) appears quite noisy for unstable and neutral conditions. I would suggest the authors remove those measurements from subfigures (a), (b), (c), (f), (g), (h), (k), (l) and (m) as it is difficult to conclude anything from them.

We agree that the coherence estimates for the 120 m and 160 m separations under unstable and neutral conditions are associated with considerable uncertainty due to the limited number of available samples. To improve the clarity of the figure, we have removed these estimates from the corresponding subfigures and updated the figure caption accordingly.

Changes to manuscript

Updated Fig. 21

Figure 21. Lateral co-coherence of (a) – (e) along-wind, (f) – (j) cross-wind, and (k) – (o) vertical velocity from SRWS for separations ranging from 10 m to 160 m under strongly unstable (left) to strongly stable (right) conditions. The 120 m and 160 m separations are omitted for $\zeta < 0.1$ due to high uncertainty associated with a limited sample size.

Referee comment

Line 402 – 403 and **465 – 466**: The primary reason one does not see attenuation is that it cancels out when calculating the coherence as a ratio of the cross-spectra to the auto-spectra. For the smaller separations, the coherence figures show appreciable coherence at normalized wavenumber below 1. So, I believe your argumentation in Lines 402-403 is not completely correct.

We agree that the normalization of the cross-spectra by the corresponding auto-spectra is the primary reason why volume-averaging attenuation has little influence on the coherence estimates. In fact, we have used this argument for both the cup-anemometer (line 417 – 419) and lidar data (line 434 – 436). We acknowledge that it was not stated explicitly here and have revised the manuscript accordingly. Nevertheless, we retain the discussion of the affected frequency range, as it provides an additional justification that any residual influence of volume averaging is expected to be negligible at normalized wavenumbers below 1.

Changes to manuscript

Line 434:
As reported by Cheynet et al. (2016a), a reduction of the lidar-based coherence estimates due to spectral attenuation related to volume-averaging is unlikely because the cross-spectra are normalized by the correspondingly attenuated auto-spectra. Any residual influence is expected to be negligible for normalized wavenumbers below 1, as the attenuation primarily affects frequencies above 0.04 Hz. For a separation of 60 m, these frequencies correspond to $k\,d_z < 1$ only for mean wind speeds exceeding $15\ \mathrm{m\,s^{-1}}$, which are rarely encountered in the present dataset.

Referee comment

Line 494: I believe there is a typo here because this line seems to contradict the statement in line 444.

We thank the reviewer for this observation. This was indeed a typographical error, and we have corrected it in the revised manuscript.

Changes to manuscript

Line 547:
Lateral coherence is systematically higher than vertical coherence, except under neutral conditions where both are of comparable magnitude.

Referee comment

Line 500: Please mention something about the future research that can be pursued to build on this study. Currently, the conclusions section is devoid of any outlook on future research.

We acknowledge that the conclusions would benefit from a brief outlook on future research. We have therefore added the following paragraph to the end of the conclusions section to highlight potential applications of the present results and directions for future work.

Changes to manuscript

Line 558:
The resulting turbulence and coherence model parameters are provided as functions of atmospheric stability in App. B and App. C, respectively, and may serve as input for the generation of synthetic wind fields under non-neutral atmospheric conditions. Future work could build on these results by investigating how the observed stability-dependent variation in turbulence characteristics influences the dynamic response and loading of wind turbines. In addition, the measured spectra provide a valuable basis for a comparison with the uniform shear model (Syed and Mann, 2024) under different atmospheric stability conditions. Further research could investigate methods for estimating lateral coherence from in-situ measurements of vertical coherence, enabling a more comprehensive characterization of atmospheric turbulence with limited measurement equipment.

Referee comment

Appendix A: I think this part can be removed because it does not add anything to the analysis. If its purpose is to support the stability classifications, I think that is unnecessary as using sonic anemometers and the Monin-Obukhov length is a well-validated method.

We agree that Appendix A is not essential to support the stability classification, which is based on the well-established Monin–Obukhov length estimate from sonic anemometer measurements. As the appendix is not directly used in the turbulence analysis, we have removed it from the revised manuscript.

Changes to manuscript

Removed App. A

Line 81:
The stability-dependent coefficients obtained by fitting empirical models to the turbulence spectra and coherence estimates are provided in App. A and B, respectively.

Line 291:
~~Further validation is provided by comparing mean wind speed profiles derived along the vertical line of the SRWS bow-tie scan (Fig. A1). As expected, stable conditions exhibit the strongest vertical shear, whereas unstable profiles are comparatively uniform.~~

3. Minor comments

Referee comment

Line 21: One could also mention that stability or non-neutral stratification affects the 2-point statistics such as the coherence.

We agree that atmospheric stability also influences two-point turbulence statistics such as coherence. However, at this point in the introduction the discussion is focused specifically on the development of auto-spectral models under non-neutral atmospheric conditions. Since the influence of atmospheric stability on coherence is introduced shortly afterwards (line 47), we have chosen to retain the current structure to avoid interrupting the flow of the discussion.

We further thank the reviewer for the following comments. We agree with the suggestions and have implemented the corresponding changes in the revised manuscript, as detailed below.

Referee comment

Line 84: during former measurement campaign → during previous measurement campaigns

Changes to manuscript

Line 85:
[...] during previous measurement campaigns [...]

Referee comment

Line 85: Weser estuary **along** the North Sea coast

Changes to manuscript

Line 86:
[...] on the east bank of the Weser estuary along the North Sea coast [...]

Referee comment

Line 86: “To the north through southeast”, please mention in the clockwise or anticlockwise direction

Changes to manuscript

Line 87:
To the north through southeast in clockwise direction, [...]

Referee comment

Line 136: 349 to 29°. I see that you define ranges in many places as x … y, please change that to x to y.

Changes to manuscript

Line 109:
[…] indicating both the estuary (315° to 345°) and the grassland southwest of the test site (220° to 250°) as the prevailing wind directions.

Line 138:
[…] mean wind directions of $\bar{\phi} = 349°$ to $29°$ for the mast sensors and $\bar{\phi} = 309°$ to $69°$ for the SRWS measurements.

Line 144:
[…] the availability is slightly higher (68 % to 71 %) […]

Table 3:
10 to 160

Figure 8 (caption):
Data within 270° to 315° is excluded […]

Line 281:
reaching a maximum of approximately 70 % at $\bar{u} = 10\ \mathrm{m\ s^{-1}}$ to $11\ \mathrm{m\ s^{-1}}$, […]

Figure 23 (caption):
[…] for separations of $d_j = 10\ \mathrm{m}$ to $40\ \mathrm{m}$ and mean heights of $\bar{z} = 120\ \mathrm{m}$ to $130\ \mathrm{m}$.

Line 490:
[…] within the range of $\bar{z} = 120\ \mathrm{m}$ to $130\ \mathrm{m}$ are included.

Line 507:
[…] up to approximately 2 Hz to 6 Hz […]

Line 531:
Under convective conditions, integral length scales of $L_u = 100\ \mathrm{m}$ to $300\ \mathrm{m}$, $L_v = 100\ \mathrm{m}$ to $200\ \mathrm{m}$, and $L_w = 20\ \mathrm{m}$ to $100\ \mathrm{m}$ are observed […]

Line 536:
[…] the integral length scales decrease substantially to approximately $L_u = 40\ \mathrm{m}$ to $110\ \mathrm{m}$, $L_v = 10\ \mathrm{m}$ to $50\ \mathrm{m}$, and $L_w = 4\ \mathrm{m}$ to $30\ \mathrm{m}$, […]

Line 542:
This contribution is also reflected in slightly increased integral length scales of $L_u = 50\ \mathrm{m}$ to $120\ \mathrm{m}$ and $L_v = 15\ \mathrm{m}$ to $60\ \mathrm{m}$.

Referee comment

Line 195: Please use another terminology for ε as “yaw misalignment” can be confusing to many readers.

Changes to manuscript

Line 203:
[...] resulting from the wind-direction offset ε.

Line 207:
[...] horizontal wind directions producing a maximum offset of [...]

Referee comment

Line 354: smaller sample size. However, they seem...

Changes to manuscript

Line 373:
The SRWS spectra appear less smooth due to the smaller sample size. However, they seem to capture the main spectral features, [...]

Referee comment

Line 446: The dataset comprises of* 20 months...

We thank the reviewer for this suggestion. To the best of our knowledge, the original wording ("The dataset comprises 20 months") is grammatically correct, as the verb *comprise* is conventionally used without the preposition *of*.

References

Cheynet, E., Jakobsen, J. B., Snæbjörnsson, J., Mikkelsen, T., Sjöholm, M., Mann, J., Hansen, P., Angelou, N., and Svardal, B.: Application of short-range dual-Doppler lidars to evaluate the coherence of turbulence, Exp. Fluids, 57, 184, https://doi.org/10.1007/s00348-016-2275-9, 2016a.

Cheynet, E., Jakobsen, J. B., and Reuder, J.: Velocity Spectra and Coherence Estimates in the Marine Atmospheric Boundary Layer, Bound. Layer Meteorol., 169, 429–460, https://doi.org/10.1007/s10546-018-0382-2, 2018.

Patel, A., Mann, J., Sjöholm, M., Thorsen, G. R., Syed, A. H., Simon, E. I., Hung, L.-Y., and Gottschall, J.: Lidar observations of turbulence for tall offshore wind turbines, J. Fluid Mech., 1028, A9, https://doi.org/10.1017/jfm.2025.11103, 2026.

Syed, A. H. and Mann, J.: A Model for Low-Frequency, Anisotropic Wind Fluctuations and Coherences in the Marine Atmosphere, Bound. Layer Meteorol., 190, 1, https://doi.org/10.1007/s10546-023-00850-w, 2024.